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ABSTRACT

A discussion of parent behavior antecedents, cognitive correlates, and the multidimensionality of locus of control in young children includes reports of several different experiments. Results indicated that development of internal control expectancies is correlated with cognitive-intellectual development. Maternal behaviors were studied through a structured observation technique. The maternal behavior variable most consistently related to the child's internal-external control (IE) was "quality of the total relationship". There is fairly conclusive evidence for the efficacy of manipulating IE in early childhood through parent education and consultation programs. (SBT)

Parent Behavior Antecedents, Cognitive Correlates
and Multidimensionality of Locus of Control in Young Children¹

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There are three topics I would like to discuss, as the title of my paper suggests. The last two concern data collected the past year; but the first doesn't. It grows out of data I reported last year (Stephens, 1972a). I'd like to update some thoughts about implications of those data, because they may be the most important phenomena of all for subsequent IE research.

Multidimensionality of IE

This concerns the multidimensionality of IE. I'm referring here not so much to the results of factor analyses of single IE tests -- Rotter's (1966) adult test, or my test (Stephens & Delys, 1973) for children, or any other -- even though many of the same conclusions apply. What I'm referring to primarily is differences among tests, especially tests for children. In two studies (Stephens, 1972a, b), one with second graders and one with third graders, we have found essentially zero order, or at least quite low, correlations among my test, the Nowicki-Strickland test (Nowicki & Strickland, 1973), the Crandall IAR test (Crandall, Katkovsky & Crandall, 1965), and/or a test Gerald Gruen, John Korte and I developed (Gruen, 1970) which resembles the IAR. This at first glance looks like bad news. At second glance, though, I think it suggests some very important and fundamentally good news.

First let me say that there is an urgent need to see if the same phenomena -- very low correlations among these tests, and among other different kinds of IE tests too, for that matter -- occur in older children and adults; and there is even more urgent need to collect, in the same studies, evidence regarding differences in correlates of the different kinds of tests. I have no data with which to test what I'm about to suggest are the differences in these tests; and we clearly need such data. These data

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will be damnably hard to collect, though, since they will require a large block of test-taking time in school-age populations, and that is both expensive and very difficult to secure consent from school officials for. So in part I'm offering a plea, for anyone who might be in a position to be able to, to consider collecting data of this sort; I'd be willing -- eager, in fact -- to help in any way I can.

The good news is this. I think we have been trying to cut butter with an axe. For fifteen years, those of us who have been in IE research have, without really realizing it, been trying to talk about several different things -- different variables, different phenomena -- under the same label. They look the same, they often are functionally similar, but they are different, and importantly so. And now I think we know it; and because of that we can now be a great deal more analytic in our efforts to learn more about IE than we have been, and we should as a result begin to get more precise, replicable, and clear findings. And that should be good news.

What I'm suggesting is that IE is not a variable, but a set of variables. I don't mean just that there are different IE expectancies, or that there are situational specificity phenomena, nor am I referring to what the factor analyses have documented; I mean more than that. Two years ago Crandall (1971) pointed out that the Nowicki-Strickland test would seem, from its format and item content, to represent what might be called expectancy of competence -- (see Strickland, 1972) -- which is one aspect of IE, as we have thought about it, but which substantially overlaps Freedom of Movement. I later (Stephens, 1972a) suggested that the Crandall test would seem, from its format and item content, to represent what might be called a willingness to publicly accept responsibility for one's ^{successes and} failures and/or destiny. This is surely another aspect of IE as we have thought about it -- but different from the first. My own test would seem, from its format, to represent neither, but rather a kind of perceptual or cognitive style -- how the subject looks at or thinks about the important events in his life in terms of the contingencies for their occurrence (his own behavior or otherwise). And that is another aspect of IE as we have thought about it. Each of these tests represents a set of operations designed to elicit responses from which to draw inferences regarding a mediating variable -- the subject's IE expectancies. In our trying to find different measurement

strategies for operationalizing IE, we may unwittingly have laid bare the anatomy of the variable we have been talking about and thinking about.

It would be stupid to wonder which of these is the "real" IE. It is surprising, but fascinating rather than discouraging, to find that these variables are essentially uncorrelated, at least among the second and third graders we tested. What's really interesting is that these three variables are so very similar in terms of both their antecedents and their performance and other behavioral correlates. I'll not elaborate this point here, but each of the three tests now has a substantial research literature surrounding it, and it's striking how parallel the tests are in terms of antecedents (socioeconomic status, age, parent behavior) and their functional properties (impact on achievement, competence behaviors, etc.). But they aren't correlated! That, I suggest, is a very interesting phenomenon. And recognizing the clear differences of these three variables should help us sharpen up our thinking. Incidentally, we should get similar benefit from pursuing the implications of sex differences in IE antecedents and correlates (see, e.g., Crandall, 1973, and Nowicki, 1973): again, initially these differences seem embarrassing, or at least annoying, since they preclude the simple generalizations we'd like to make; but in the long run they will help us by leading us to a clearer understanding of the mediation of IE development and of IE-related behavior.

I think, in summary, that IE, in our thinking, has been a conglomerate of some fairly humanistic, Adlerian phenomena (e.g., "competence" expectancies) and some very antiseptic, probability phenomena (e.g., contingencies for reinforcement), among others. That has been, from the first, part of the great interest in and apparent promise of the IE construct. It is now documented that these phenomena do overlap often. But often they don't. In our various attempts to operationalize these expectancies -- to measure them -- we have unintentionally revealed some of the nonoverlap. Paradoxically, by this accident we've learned a lot about IE, in young children, at least; and this may represent a really great advance in our understanding of what is going on.

Now let me turn to some findings of the past year.

IE and Cognitive Development

This year we made an all-out effort to test correlationally whether internal control development in preschool age children is related to intellectual development, and also to cognitive development as conceptualized by Piaget. We used the full WPPSI; and Mary Jo Watson developed a battery of tasks to assess the child's attainment of seven Piagetian cognitive operations: classification, class inclusion, discrimination by number, discrimination by size, seriation, counting, and conservation. We gave these tasks, the WPPSI, and our SDRCI IE measure to 61 nursery school children, all middle class or above.

Internal control scores correlated .37 ($p < .01$) with WPPSI IQ scores and .28 ($p = .03$) with composite scores on the Piaget tasks; WPPSI and Piaget scores were correlated .49 ($p < .001$). (Incidentally, the limited chronological age range we had was correlated .20 ($p = .13$) with Piaget task scores, .27 ($p = .04$) with IE, and $-.15$ ($p = .25$) with WPPSI IQ scores.) IE scores were not very much more highly correlated with Verbal (.36) than with Performance scale (.28, $p = .03$) scores on the WPPSI; and we were happy, if a bit surprised, about this, since this argues against interpreting the results as reflecting simply that scores on our test are increased by greater verbal abilities. (I should report, too, that there were some sex differences: for boys the correlations with WPPSI and Piaget task scores were lower than for girls and nonsignificant, though still positive.) There really was no decipherable pattern regarding which WPPSI or Piaget tasks were more and which less related to IE. There were surprisingly low intercorrelations (see Table I) among WPPSI subtests -- generally only in the .30's; and this clarified our findings of the previous year in which we used various short-form intelligence measures and found very little correlation among them.

In any case, with this as well as the less conclusive data from previous studies with short form intelligence tests, (see Stephens, 1972a) it can now be concluded pretty confidently that, among nursery school children, development of internal control expectancies is correlated with cognitive-intellectual development. The key question now is whether (1) this means faster development of internal control expectancies may expedite intellectual development, as we hope and suspect may be the case; or (2) whether the relationship is the reverse, with more intelligent children learning internal

control expectancies more rapidly than less intelligent children; or (3) both -- or (4) neither. There isn't space in this paper to review the theoretical rationale for the hypothesis that internal control expectancies may help mediate intellectual development in early childhood or the IE data which supports this rationale. This was reviewed in my paper last year (Stephens, 1972a). The safest expectation may be that each -- IE development and intellectual development -- tends to enhance the other, in a reciprocal rather than unidirectional "causal" chain. This would still indicate that, if ways can be found to experimentally increase rate of internal control expectancy development in young children, a by-product of such manipulations would be to enhance cognitive development; and, in turn, such a finding would more strongly identify internality as being an antecedent, even if it may also be a partial consequent, of intellectual development.

Parent Behavior and IE Development

The major effort of my own research now is to identify the maternal behavior antecedents of internal control development. The plan is to try first to identify maternal behavior characteristics that are correlated with internal control development, and then to test experimentally whether these may indeed be antecedents of internal control development. Again, the maternal behavior correlates we find could be either antecedents or consequents, or both, of the child's IE development: watching some 80 or 90 mother-child dyads interacting in a structured situation has impressed us with the impact of the child's behavior on the mother's behavior. (Still, generally it appears that the maternal behaviors we have observed seem more likely to be antecedents of than responses to the child's IE-related behaviors.) We plan, then, to try to manipulate these maternal behavior characteristics, in a subsequent project, by means of parent education and consultation programs, and then assess the impact of these programs on the child's IE development -- and, as I said earlier, on the child's cognitive development.

To assess maternal behavior, we adapted the technique used by Solomon, Houlihan, Busse, and Parelus (1971) and Hess and Shipman (1965) and others. We could not afford home visitor observations. But, most crucial, we wanted to identify specific kinds of behavior we could teach

mothers; so we wanted to be able to see the behavior, and interview or questionnaire data wouldn't allow that. So we set up a standard, structured situation in which mother and child interacted, and we directly observed their interaction -- about 20 minutes' worth. We also videotaped the interaction; and this was crucial, because I think we have learned most by being able to replay tapes over and over again so we could revise and refine the list of maternal behavior variables to score. The situation in which we observe the mother-child interaction is one in which mother is teaching or helping, or just working with, the child on various tasks: an Etch-a-Sketch design drawing task, a practically insoluble "Instant Insanity" -type puzzle, a bean bag throwing task, and a Tinker Toy construction task. We do the observing in the nursery school where the child is enrolled, and mother and child are fully aware they're being observed: the camera is in full view, and so are we. Generally we give the instructions not to the mother but to the child: we tell him, "You can ask your mother for help if you want," but this fails to restrain a lot of mothers from unsolicited help.

I think it is important, especially in light of Dr. Crandall's paper (Crandall, 1973) to follow, for me to emphasize one particular aspect of our procedure: how we decided on what maternal behavior variables to measure. What we did was heterodox; but I believe, if I may explain the rationale, it was methodologically sound. We decided not to be guided, initially at least, by either previous studies or by a priori theoretical hypotheses regarding antecedents of IE. Our purpose was not to test hypotheses about IE development, but to find ways to manipulate IE development so as to test its relation to cognitive development. Previous studies had dealt with older children, and used other IE tests, and most had not focused on specific, teachable maternal behaviors of the sort we hoped to find. We decided to use previous studies and theoretical deductions as the second step in generating ideas as to what kinds of maternal behavior to investigate. The first step was, in effect, to let our subjects teach us what to look for. We simply looked at all the tapes a time or two each to see in what ways the mothers differed from one another, relatively clearly or dramatically, that might be important in influencing the child's personality development. From this we generated a list of variables. We then did more pilot work to see which of these variables would permit reliable measurement, which could not be distinguished from which others, what explicit definitions and scoring rules we needed to

sample of 31 mother-child dyads on these variables, and formally assessed rater reliability, overlap between variables, etc. Then, on the basis of this experience, we completely reorganized the list of variables to measure -- dropped many, added many, redefined many; and we rated this year's sample, again 31 dyads, and then re-rated last year's, on the revised list. Then, and only then, we looked systematically at past studies and theoretical suggestions for variables to add to the list. As it turned out, there were few, if any, to add: the variables we saw that might be added either couldn't be scored from our data (because they weren't sufficiently "behavioral" or just because they didn't happen in our samples) or were near-equivalents to variables we already had rated. The point of all this is this: that, where our data are consistent with other studies, they really are^{as} independent as possible a data base: we have a different age group, IE test, and method for assessing parent behavior, and our study was purposely as independent as possible regarding the basis for generating hypotheses.

What we ended up with was a set of seven classes of variables; and what we found was not, frankly, what we had expected. Specifically, we had four classes of mother behavior variables; two classes of mother-child interaction pattern variables; and a set of child-behavior variables (see Table II). Of the mother behavior variables, the first class was a set of specific, countable behaviors: the number of times she encouraged the child, or gave an overt direction, or gave some information, etc.; these we scored simply by frequencies. The second class was essentially the same kind of behavior, but rated on a 1-7 point scale -- encouragingness, directiveness, etc.; we did this to see how much we might lose, by being less objective, or gain, by allowing account of qualitative differences (e.g., between a mild suggestion and a categorical command); as it turned out, we got very high correlations between the frequency counts and more qualitative ratings of the corresponding kinds of mother behavior, and neither seemed to give clearer results than the other. A third class was non-countable mother behavior characteristics, but still characteristics that required little inference to rate and had high rater reliability -- for example, attentiveness, or achievement orientation. The fourth class was more emotional than behavior in character -- tense-relaxed, and cold-warm. Of the interaction pattern variables we had one set of relatively "behavioral"

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patterns -- cooperativeness, degree of power struggle, and child's acceptance of mother's help and suggestions -- and, as a class unto itself and just for the heck of it, we rated just the "overall quality of the relationship". Of the child-behavior variables I'll speak more in a minute.

What we found was completely unexpected: the more inferential the variable was, the farther it was from specific behaviors of the mother, the more consistently and strongly it was correlated with the child's IE (see Table II). The data are analyzed separately for boys and girls, since sex differences in parent behavior correlates of IE have been quite common in the literature (see Crandall, 1973), and separately for last year's and this year's samples (partly because of some changes in procedure made this year); so the correlations are based on samples of only 15 or 16 per sub-sample. This lets us look, though, at replicability and cross-sex generality of our results. For single samples the degrees of freedom are so low that there is almost a prohibitively high correlation needed to reach, for example, the .05 significance level; but it is appropriate to base conclusions on combined probability levels, and to check for consistency in direction and magnitude, if not significance, across the separate samples.

In any case, the variable most consistently and clearly related to the child's IE is "quality of total relationship" -- which turns out, surprisingly, to be pretty reliably rateable. Nearly as good are the correlations of IE with maternal emotional variables -- cold-warm and tense-relaxed. Least relation to IE tended to come from the most discrete behavioral variables: there were fairly consistent tendencies for child IE to be related to attentiveness and -- the only "countable" behaviors related to IE -- to mother's tendencies to give discouragements and to give overt directions to the child (correlated with more external control scores for the child.)

There was also another surprise. We rated several child variables too, hoping that we might eventually have a big enough sample that we could break it down into different "kinds" of children -- active vs. quiet, for example -- to see whether the same mother behaviors had different impact on different kinds of children. We don't have enough subjects for this yet. But we find that most of these child behaviors we picked out are themselves fairly

consistently related to IE. And, of all things, one of the most consistent is again, the most inferential: judges' ratings of the child's self-concept. Other variables consistently related to internality were activeness and dominance; and a couple of others -- initiative and task-orientation -- were fairly consistently related to internality too.

These data are of interest in two ways. First, they are notable simply as correlates of internality in young children. Note that these data are completely separate from the data base for the IE scores -- our SDRCI interview measure (Stephens & Delys, 1973). And note that there would not seem to be anything in our IE measure to make these correlates simply an extension of the content of the IE measure, or otherwise artifactual. The second relevance of these data is a little more subtle. Initially it seemed to be a disappointing irony that we were looking for mother-behavior correlates of IE, so that we might be able later to manipulate them and thus enhance IE development, but what we found especially was a lot of child-behavior correlates of IE. But this may be more fortuitous than it looks. Although the reasoning may seem familiar, it is reasonable to suspect that there may be a reciprocal relationship between internality and such behavioral dispositions as these -- activeness, initiative, dominance, task-orientation (or even "self-concept"). Increase internality and, as a lot of previous studies indicate, one might well expect -- in general, ceteris paribus, etc. -- these behaviors to increase; but also, the more the child behaves thus, the more he is likely to encounter experiences from which he is likely to develop internal control expectancies. Now, if this is so (and it is testable), it leads to another possibility. Mothers -- or teachers, or whoever -- can't reinforce internal control expectancies directly, since they aren't behaviors but mediating processes. But they can reinforce these behavioral correlates. It may well be that among the most effective ways mothers do enhance development of internality -- and/or can be taught to -- is, simply, to reinforce or otherwise encourage these behaviors and thus, indirectly, internality.

In any case, these findings are strikingly consistent both with the data Dr. Strickland is going to summarize (Strickland, 1973) concerning the "competence behavior" correlates of IE, using mostly the Nowicki-Strickland IE test and older subjects, and with the data Dr. Crandall will summarize

(Crandall, 1973) regarding parent behavior antecedents, using mostly the Crandall IAR IE test and, again, older subjects. And it does look as if there will be a fairly solid basis for trying to manipulate IE in early childhood through parent education and consultation programs -- and thereby test experimentally both the parent behavior antecedents of IE and the role of IE in cognitive development.

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